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Interactive comment on "Aggregation effects on tritium-based mean transit times and young water fractions in spatially heterogeneous catchments and groundwater systems, and implications for past and future applications of tritium" *by* M. K. Stewart et al.

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Dear Referee, dear authors, dear editor,

I would like to react to a couple of points that have been raised by referee #1.

Firstly, I was very surprised to read in comment 1 that referee #1 is explaining the transit time distribution by the presence of heterogeneities. This is plain wrong for the exponential model, which describes the TTD of a homogeneous semi-confined aquifer

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exactly, and where the distribution is due to the different path lengths in the catchment, and isnot accurate at all for the dispersion model and the gamma model either, both of which include dispersion and diffusion, but ALSO the distribution of pathlength. The distribution collapses to a piston flow ONLY if all flow lines can be assumed to be of equal length. In the rest of that first comment, referee #1 seems to confuse two related issues: complex systems and heterogeneous systems. In a complex system, different and conceptually clearly separate reservoirs sustain discharge at the system's outlet. Usually, hydrogeological understanding can lead to a choice of LPM combination which best simulates that system (say two exponential components for the quickflow and baseflow reservoirs), or the fit improves significantly by doing so. This assumes that each reservoirs is sufficiently homogeneous to use a given LMP shape, all of which have been developed for a homogeneous medium. Piotr Maloszewski and co-authors have over the years shown quite a few example of improving the fit to measured tritium activity by combining models and hydrogeological understanding. Kirchner's model is somewhat similar in that TTDs are added up to simulate a heterogeneous system. This is not the same however, first because one would expect a heterogeneous catchment to be made up of more than two or three subcatchments (and hence two or three TTDs) which flow into one another, and second because while this combination is a conceptual contrivance, the combination used in complex systems is a conscious decision of the experimenter made based on data. The last sentence of that comment is not clear to me. What does the referee mean by "a specific set" ? What other set of heterogeneities does he think about ? Maybe he is pointing to the shortcoming of the model I was writing about in my comment.

In comment 2, the confusion between complexity and heterogeneity I mention above appears again. I think it is useful to clarify what is happening to the TTD in heterogeneous catchments (this is what we have tried to show, Piotr Maloszewski and I, in our manuscript currently under review at HESS). If the MTTs of the subcatchments are very different (say 60 days and 5 years) and assuming exponential TTDs, then the total TTD will be more curved than an exponential. So fitting an exponential model to the

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tracer output of that system will result in a parameterised TTD that underestimates the younger fraction and often overestimates the older fraction to compensate (I suppose this is what the referee means with "a single LPM would results in a strong bias"). But since the modeller in a real world case has no access to the true TTD (which is more curved than the one he fitted), he will never know, and will probably not notice unless the output time series is exceptionally long and the fit bad. Referee #1 seems to suggest that we can guard ourselves against this problem by always using combined LPMs. This is not really the case. First, it can well be that the true TTD is so curved that even a double LPM will deviate from it significantly. And second, this increases the number of free parameters, in many cases beyond what can be robustly fitted to the data available. So no, using "compound LPMs" does not mean we get rid of the "aggregation effect".

I also disagree with comment 6 and would support the approach adopted by Stewart et al.. I think hydrologists should stop fitting blindly all kinds of models to output time series and look at exotic measures of fit to decide what model is "best". Stewart et al.'s case by case analysis is done with system understanding in mind, and emphasizes the need for the modeller to ask himself whether a model improvement makes sense physically.

The referee may not know previous work by Mike Stewart and Uwe Morgensten showing that the "ambiguity" he alludes to in comment 8 disappears once the memory of the bomb peak has "faded" from a groundwater system. Of course, as long a the tail of the bomb peak is measurable, more information can be gained from it. In particular, it is sometimes possible to recognise the need for a compound LPM, which becomes impossible using a single post-peak value (this has also been recognised by Stewart and Morgenstern). All this is not a matter of opinion, but is firmly rooted on established results.

As for the specific comments, number 4 raises the question of what IS a realistic difference in MTTs. In my comment I contend that answering this question is very problematic as long as the problem is posed in terms of MTTs instead of hydraulic variables (whose range of variation can be measured in the field).

Best regards.

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